

95400

(P)

61389/10

ERSTON, J.A.



Digitized by the Internet Archive  
in 2018 with funding from  
Wellcome Library

<https://archive.org/details/b30380248>

*Dr. George was very kind  
wishes.*

OBSERVATIONS

ON THE

ACTION OF THE ACETATE OF POTASH

IN SOME

DISEASES OF THE SKIN.

BY

J. A. EASTON, M.D.,

ONE OF THE PHYSICIANS TO THE GLASGOW ROYAL INFIRMARY,  
LECTURER ON MATERIA MEDICA IN ANDERSON'S INSTITUTION, GLASGOW, ETC.

EDINBURGH: SUTHERLAND AND KNOX.

LONDON: SIMPKIN, MARSHALL & CO.

95408



[FROM THE MONTHLY JOURNAL OF MEDICAL SCIENCES, FOR MAY 1850.]

## OBSERVATIONS.

---

(Read before the Glasgow Medical Society on 19th March 1850.)

---

IN submitting to the Society the following observations regarding the acetate of potash in the treatment of certain diseases of the skin under my charge in the Royal Infirmary of this city, I beg to state, in the outset, that I was led to employ that remedy from reading the lectures of Dr Golding Bird, delivered before the Royal College of Physicians of London in April 1848, and reported in the 6th and 7th volumes of the New Series of the "London Medical Gazette." I am anxious also to impress upon my professional brethren, that the speculations in the following pages regarding the *modus operandi* of that salt in the diseases in question—those at least which are entitled to consideration and deference—have likewise emanated from the same distinguished medical philosopher; and that, where I have ventured on a modification or more extensive application of his views, I have endeavoured to keep within the limits of established doctrine, or, at all events, of what appeared to be permissible theorising and legitimate inference.

The following cases, out of several in which the acetate of potash was used, have been selected as presenting the skin diseases most commonly met with, and they include *Psoriasis diffusa*, *Psoriasis palmaris*, *Lepra vulgaris*, *Eczema rubrum*, and *Eczema impetiginodes*. To save the wearisome repetition of almost similar details, I shall abridge from the journals of the hospital, the reports which were taken on the admission of the patients by my intelligent clerk, Dr James Howie, and shall then annex a short summary of the prominent circumstances in the progress and result of each case.

CASE I.—*Psoriasis diffusa*.—"Ann M'Intyre, æt. 16, servant, admitted 18th September 1849. About five months ago, without assignable cause, patient observed some small red patches on lower extremities, which were soon covered



by whitish scales, which fell off, leaving the surface red, shining, and tender to the touch. The patches gradually enlarged, till at length they formed an almost continuously affected surface. Two months after patches had appeared on lower extremities, some were observed on the upper; these soon extended, so as to cover the arm from the wrist to above the elbow. On admission, the upper and lower extremities are found to have the usual appearance of psoriasis. Elbow, wrist, and knee-joints, but especially the last, are deeply fissured, and covered with thick yellowish crusts. One or two patches have also appeared on the face." *To use the alkaline warm bath every day; to take five grains of the Iodide of Potassium thrice daily, and a Plummer's pill every night.* Under this treatment the disease continued to increase in intensity and diffusiveness till the 13th October, when she was ordered the following mixture:—*Acetate of Potash* ℥v., *Water* ℥xij., *Mix.*; of this mixture take an ounce thrice daily. *Omit the other medicines.* Was dismissed cured on 7th December. Duration of disease from commencing acetate of potash less than two months. Let it be noticed, that in this case iodide of potassium and Plummer's pill, though persisted in for nearly a month, failed to make the slightest impression on the disease. No record was kept of the state of the urine.

CASE II.—*Psoriasis diffusa*.—"Sarah Miller, æt. 18, factory worker, of full habit of body, admitted 19th September 1849. About two months ago an eruption of small red patches, covered with white scales, appeared on lower extremities, and three weeks ago on upper extremities. The lower extremities present the usual appearance of psoriasis diffusa, and the eruption is not so general or intense on upper extremities. Skin hot; pulse 72, full." She was ordered the warm bath, and to take 4 ounces of the following mixture morning and evening:—*Nitrate of Potash*, 2 drachms; *Sulphate of Magnesia*, 1 ounce; *Water*, 24 ounces—*Mix.* On the 24th September, she was put on the acetate of potash, and the diuretico-cathartic mixture was discontinued. She was dismissed cured on the 14th November, the disease having lasted nearly two months from the period when the use of the acetate of potash was commenced. No record was kept in this case either of the condition of the urine.

CASE III.—*Psoriasis diffusa*.—"Margaret Lindsay, æt. 40, laundry-woman, widow, admitted 26th November 1849. On admission, the whole dorsum of left foot and lower half of leg are covered with a scaly eruption, having numerous fissures. The scales being removed, the subjacent surface presents a red shining appearance, and the scales are quickly reproduced. The right leg and both arms are similarly affected, but in a milder degree." Was ordered an ounce three times daily of the following mixture:—*Acetate of Potash*, 6 drachms; *Water*, 12 ounces—*Mix.* Patient was cured of the psoriasis in less than four weeks, but remained in the hospital a fortnight longer in consequence of cephalalgia. Average quantity of urine passed per diem  $7\frac{1}{2}$  pounds—16 ounces to the pound. Average sp. grav. 1.016.

CASE IV.—*Eczema impetiginodes*.—"Margaret M'Callum, æt. 21, unmarried, bleacher, admitted 19th January 1850. Two months ago, after some pyrexial symptoms, patient observed a vesicular eruption on upper extremities. It was attended with some itching, and when scratched, gave exit to a clear fluid, which concreted into yellowish crusts. Of late, however, the eruption has assumed a pustular character. On admission, the skin between wrist and elbow-joints, and on forehead and cheeks, is covered with a continuous yellow crust. Pulse 116, full; tongue dry; skin hot." This patient was first bled to 12 ounces, and had a smart purge of calomel and jalap. The blood drawn was buffy. On the next day (the 20th), the pulse being 96, and the febrile symptoms considerably abated, she began the use of the acetate of potash in half-drachm doses thrice daily; and I may here mention, that this is the quantity which I now invariably prescribe to adults. This woman left the hospital cured on the 23d of February, having been under treatment by the acetate one month and three days. It is of consequence to note that the urine voided



during the first twenty-four hours after admission, and before any medicine was given, was only eight ounces, high coloured, and of sp. grav. 1·032 ; the average amount under the use afterwards of the salt, was 54 ounces, of average sp. grav. 1·019. In reference to this case, it is but fair to mention, that within the last few days I have been informed by my friend Mr Menzies, one of the surgeons to the city poor, that the disease re-appeared a short time after the patient left the hospital.

CASE V.—*Eczema impetiginodes*.—"Rachel M'Allister, æt. 31, single, dye-worker, admitted 19th January 1850." This case was so similar to the foregoing, that I think it unnecessary to transcribe the details *ad longum*. There was no febrile exacerbation, however, in this case as in the former, the pulse on admission being 84, the tongue moist, and the skin cool. Amount of urine voided during the first twenty-four hours—no medicine having been given—was 32 ounces, of sp. grav. 1·013. She was put on the usual treatment, and, subsequently, the average quantity of urine was 78 ounces, of average sp. grav. 1·016. She left the hospital all but cured on the 2d March.

CASE VI.—*Psoriasis, with Extensive Desquamation of Cuticle*.—"William Dorrington, æt. 28, tailor, single, admitted 14th December 1849. Patient was treated in this house four years ago for an affection similar to the present. On admission, the skin of face is dry and covered with white scales, giving the appearance as if a quantity of flour had been applied. There is also extensive desquamation on back and arms, the lower extremities from knees downwards being red and somewhat swollen. Gums slightly tender, which patient ascribes to the slight use of mercury before admission." As this man was considerably debilitated, I deemed it advisable to improve his general health before having recourse to the acetate of potash. Accordingly, for a few days after admission, he was ordered nutritious diet, and the citrate of iron and ammonia. His condition being ameliorated by these means, he began the use of the acetate of potash, combined with infusion of gentian, which treatment was continued till he left the hospital cured on the 23d January 1850. Duration in hospital, one month and nine days. No record kept of urine. When this patient was treated in the hospital for a similar affection four years ago, he remained there for thirteen weeks.

CASE VII.—*Eczema rubrum*.—"George Morris, æt. 39, weaver, married, admitted 29th November 1849. A small patch of vesicles is situated on the inside of right forearm a little above the wrist. This eruption appeared about two weeks ago, and is now attended with considerable prurigo, and when scratched, gives exit to clear fluid. An eruption of about twelve months' standing is situated on lower extremities, from a little below the knee downwards. This is stated to have commenced in exactly the same manner as the eruption did on the arm ; at present it has a bright red appearance, and the affected surface, especially if scratched, exudes a clear fluid. General health good." Acetate of potash was administered as usual, and the amount of urine, which, during the first twenty-four hours after admission, amounted to the unusually large quantity of 4½ pounds, or 72 ounces, speedily and steadily increased till on one occasion it rose to as high as 11 pounds, or 176 ounces. Under the use of the salt the average quantity was 120 ounces, of average sp. grav. 1·012. It ought to have been stated that the sp. grav. of the 72 ounces voided before any medicine was given, was 1·016. This man was dismissed cured on the 22d December 1849, having been in the hospital three weeks and two days.

CASE VIII.—*Psoriasis palmaris*.—"Michael Dyer, æt. 14, admitted 29th January 1850." This was a case of psoriasis palmaris of five months' duration, and is presented merely because the patient, being a boy of only fourteen years, received the acetate of potash in smaller than the usual quantity, the medicine having been given in doses of 10 grains thrice daily. The average quantity of urine voided under the influence of the salt, was 56 ounces, of average sp. grav. 1·017. He remained in the hospital five weeks, and was then dismissed cured.



CASE IX.—*Lepra vulgaris*.—I saw the following case only once, and for the subjoined account of its history, treatment, and result, I am indebted to Mr T. D. Buchanan, druggist, Argyle Street, at present a student in the hospital. “J. B., aged 15½ years, flesher’s apprentice. States that five years and eight months ago an eruption appeared on hands and head, at first red, and afterwards covered with white shining scales. He was put on the use of vegetable tonics without benefit, afterwards sent to sea-bathing, where he remained only a few days, until he was completely covered with the eruption. He was then removed to town, and placed under the care of a medical man, who prescribed the warm bath every night, and the use of various ointments, some of which were supposed to contain some preparation of mercury. This treatment had little or no effect. The bed-clothes were filled with white scales in the morning. After a considerable interval, he commenced the use of Fowler’s solution, and continued it until he had taken several ounces, with no benefit. Eight months ago he began to use the iodide of starch, and continued it for about four months with slow improvement. About the beginning of October last the appearance of his hands and face was so repulsive, that he was about to be dismissed from his employment. About the beginning of December last (1849) he was seen by Dr Easton, who suggested a trial of the acetate potassæ. The suggestion was adopted, the boy began gradually to improve, and by the end of January the eruption had entirely disappeared, and has shown no symptoms of return up to this date, 16th March 1850.”

On reviewing the details of these cases, the first thing I think worthy of being noticed, is the comparative shortness of the duration of the individual diseases. The inveteracy of skin diseases in general is proverbial; and it would be easy to show, were it not superfluous, that the duration of the foregoing cases affords a pleasing contrast to that of many others of a similar kind treated by agents as diversified as numerous. Any remedy, therefore, which promises not only to remove such diseases, but to do so in a much shorter period than is usually required to accomplish that object, cannot fail to recommend itself to every enlightened practitioner, as being at least worthy of a trial. It is of consequence to remark farther, that the benefit produced in these cases was entirely attributable to the acetate of potash. As I was anxious to put the properties of this salt, as a therapeutic agent, to the most rigid test, I purposely avoided the internal use of any remedy but itself. No doubt the alkaline warm bath was had recourse to in most of the cases every day, and the affected extremities were frequently fomented. The co-operation of these valuable auxiliaries was, I admit, availed of pretty generally—in the acuter cases always; but with the exception of the blood-letting in the case of the woman M’Callum (No. IV.), of the cathartico-diuretic mixture, given for a few days at first, in the case of the girl Miller (No. II.), to subdue general plethora, and of the adjuncts of iron and gentian, in the case of the man Dorrington (No. VI.), to remove if possible a very opposite condition,—with these exceptions, I repeat, no other medicine was administered than the acetate of potash.

Having thus noticed these two circumstances, which lie on the surface, so to speak, of this investigation, I next proceed to consider the more prominent effects of the medicine, and how far these may be held to indicate its physiological action, and to explain its remedial effi-



cacy. In the first place, the administration of the salt was followed by a great increase in the amount of urine. Making allowance for difference in season, difference in the state of the atmosphere, and for other causes which are known to influence the secretion of urine, I need scarcely remind you, that the average quantity voided during twenty-four hours,—and I certainly do not take the lowest standard—is about 40 ounces, or  $2\frac{1}{2}$  pounds. Now, the smallest amount of urine which was voided in the cases under review, was 54 ounces, as in case IV.; while the greatest was  $7\frac{1}{2}$  pounds, or 120 ounces, as in cases III. and VII.,—there having been, in the case in which the amount was lowest, an increase of 14 ounces; and in those cases in which it was highest, an augmentation of 80 ounces over the ordinary quantity. These facts speak conclusively, I think, as to the diuretic power of the acetate of potash. But the term diuretic is of vague signification,—at all events, two very different results follow the administration of different classes of remedies, each of which is comprehended under the general appellation of diuretics. Thus, some of these increase merely the amount of the *water* of the urine; others, the amount of its *solid constituents*, urea, lithic acid, and the salts. Under the former may be ranked broom, juniper, squill, digitalis, and, to use the language of Dr Golding Bird, “all those agents which out of the body exert no chemical effect on animal matter.” Such substances, which Dr Bird calls renal hydragogues, appear to influence exclusively the Malpighian bodies of the kidneys,—that delicate expanse of capillaries to which, as has been shown by Mr Bowman,<sup>1</sup> has been entrusted the duty of separating the water that is discharged from the blood by the renal glands. I am of opinion, then, and the facts submitted warrant the conclusion, that the acetate of potash increases the water of the urine,—in other words, that it acts as a renal hydragogue. But it performs, when administered, a more important part in the animal economy, and renders more essential service in the removal of disease than what is imparted by its simply hydragogue influence; and this leads me to consider another effect of the salt, which I am anxious to call attention to, as having been manifested in the cases under consideration.

II. It was formerly remarked, that some of the medicines called diuretics increase the quantity of the solid constituents of the urine; and I now proceed to show, in the second place, from the cases under review, that the acetate of potash possesses this property in so remarkable a degree, as fully to entitle it to be considered a renal alterative or blood-depurant, as well as a renal hydragogue. The elimination from the blood of the solid constituents found in the urine may be considered the function of the kidney *par excellence*; for, while this gland undoubtedly separates superfluous water from the circulating mass, it cannot be said, if I may be allowed the

---

<sup>1</sup> Philosophical Transactions for 1842.



expression, to possess a monopoly in that respect, seeing that superfluous water is likewise discharged by the lungs and the skin. But it is specially—I may say exclusively—the office of the kidney, as well to de-acidify the blood, as to rid it of redundant nitrogen,—effecting the former of these objects by loading the urine with acid salts; the latter, by elaborating and excreting urea and lithic acid. Accordingly, the amount of these nitrogenous compounds in the urine, both of which, according to Dr Bird, are produced from the metamorphosis of nitrogenised elements of worn-out tissue or mal-assimilated food, may be assumed as a measure of the amount of the destructive assimilation of nitrogenised tissues or other matters in a given time; for it may be regarded as a general exceptionless fact, and be enunciated with the emphasis of a general law, that all soluble matters in the blood, which are either not able, or are not needed, to contribute to the growth and reparation of the tissues, are ejected by the kidneys. Now, while the investigations of Mr Bowman<sup>1</sup> have shown, as already stated, that the Malpighian capillaries separate the water of the urine, it has also been established by the same distinguished physiologist, that the separation of the solid constituents, or what may be called the peculiar urinous principles, is effected by the cells which line the tortuous uriniferous tubes, exercising their secreting power for that purpose on the blood of the portal capillary plexus, with which the bodies of these tubes are in intimate contact.<sup>2</sup> The amount of these urinous principles influences, of course, the density of the urine, and while the exact amount of them which is voided in a given time can only be determined by a quantitative analysis of that fluid, still, by attending to its specific gravity, we can ascertain whether they be above or below the normal amount. The specific gravity of water being estimated at 1·000, that of healthy urine ranges, as is generally understood, between 1·015 and 1·025; but, from a very painstaking and very extensive observation of this matter in our hospital, my friend and colleague, Dr Mac-Gregor, whose original researches into the pathology of diabetes entitle him to all credit for accuracy and trustworthiness, has come to the conclusion, that 1·016 is the number which expresses more

---

<sup>1</sup> Philosophical Transactions for 1842.

<sup>2</sup> The views of Mr Bowman, regarding the separate functions performed by the Malpighian bodies, and the portal capillaries which issue from them and invest the tubes, are supported by the anatomical arrangement—first pointed out, I believe, by that gentleman—of the renal vessels in the *boa constrictor*. —“Philosophical Transactions,” 1842. Additional evidence of the accuracy of Mr Bowman’s opinion has been adduced incidentally in a paper, by Dr George Johnson, in Vol. XXX. of the “Medico-Chirurgical Transactions” (1847), in which Dr J. remarks:—“Within the last two months, I have examined the kidneys of two persons, who died jaundiced, and in whose urine there had been a quantity of bile. Many of the urinary tubes were stained of a deep yellow colour, by the bile in their epithelial cells. This yellow colour ceased abruptly at the neck of the Malpighian bodies, and in no instance did I observe it in the Malpighian bodies themselves.”



correctly than any other the specific gravity of healthy urine. To the correctness of this statement my own little observation of this point leads me to subscribe. If, then, 40 ozs. of urine, of sp. grav. 1·016, be voided on an average in twenty-four hours, is there any mode short of performing a quantitative analysis—a proceeding not likely, for many reasons, to be had recourse to generally—by which we can estimate, more precisely than by merely contrasting the difference between 1·000 and 1·016, the waste of tissue which has taken place during that period in the noiseless laboratory of the organism? On this point Dr Bird supplies information so practically important that I make no apology for quoting from him nearly *verbatim*. After urging the necessity of ascertaining the integrity of the blood-depurating function of the kidney, and acknowledging, at the same time, the practical difficulties of doing so by evaporating the urine to as dry an extract as can be obtained, he calls attention to the advantages presented by the more rapid and easy determination of the quantity of solids from the specific gravity of the urine, and then subjoins a table, which he has constructed for the purpose of ascertaining that point. The peculiarities and the mode of applying this table to practical purposes, will be best gathered from the following sentences:—“Thus, if the specific gravity of any specimen of urine be expressed in four figures, the two last will indicate the quantity of solids in a fluid ounce of the urine, within an error of little more than a grain, when the density does not exceed 1·030; above that number, the error is a little greater. To illustrate this, let us suppose we are called to a patient, the integrity of the depurating functions of whose kidneys we are anxious to learn. The quantity of the urine excreted in twenty-four hours amounts, we will suppose, to three pints, or sixty ounces, and the density of the mixed specimens passed in the time alluded to is 1·020; now, we merely have to multiply the number of ounces of urine by the two last figures of the specific gravity, to learn the quantity of solids excreted, or  $60 \times 20 = 1200$  grains.” Now, bearing in mind the characteristic function of the kidney, and that the measure of its activity as a blood-depurating organ may be calculated by the specific gravity of the urine, let us see how far the function in question was influenced by the acetate of potash in the cases under consideration; in other words, to what extent the *solid constituents*, as well as the *water*, of the urine, were increased by that salt. In the cases in which the least urine was voided—being 54 ozs. in the lowest (Case IV.), and 56 ozs. in the second lowest (Case VIII.), the specific gravity was respectively 1·019 and 1·017; while in those in which the amount was highest, the specific gravity was 1·012 in one of them (Case VII.) 1·014, in the other (Case III.)—the average quantity voided by each of these patients having been 120 ozs. To those who take but a superficial view of this matter, the results just stated in reference to the specific gravity in these cases—particularly in reference to that of the two last—may appear inconsistent with the statement



that the acetate of potash increases the solid constituents of the urine, and thereby promotes the depuration of the blood. If such be the case, why, it may be asked, is the specific gravity not above the normal standard, instead of being below it? A moment's reflection will explain the reason, and show that the salt under consideration acts powerfully in promoting the elimination of those peculiar urinous principles which result from the secondary destructive assimilation of effete tissue, mal-assimilated food, and all abnormal products of low vitality and imperfect organisation. In all the cases the amount of water holding these solid constituents in solution was materially—in Cases III. and VII. enormously—increased; and it must never be forgotten that, if the *same amount* of solid matter be dissolved in a larger or smaller quantity of water, the specific gravity will be proportionally lower or higher. Thus, when the menstruum is much increased, the specific gravity may not be above the normal standard, and yet the activity of the kidneys, as blood-depurating organs, may be very great; for with the increased amount of water, there is washed out from the blood an increase of the solid urinous constituents also; and yet, as these are not in a comparatively concentrated solution, but diffused through a large amount of menstruum, the density may relatively be not higher than usual—nay, it may even be lower. Connecting this fact, then, with Dr Bird's table, and keeping both steadily in view, we shall find that, when 54 ounces of urine were voided, of the sp. grav. 1.019, as in Case IV., the blood was depurated to the extent of 991 grains in twenty-four hours; while, when 120 ozs. were passed, of the sp. grav. 1.011, as in Case VII., the separation of salts and waste of tissues, as indicated by the criterion of the specific gravity, amounted to 1440 grains in the same period. These results, then, furnish, I think, abundant evidence of the power which the acetate of potash possesses in promoting the metamorphosis of tissue, thereby increasing the solid constituents of the urine, and proportionally depurating the blood.

III. But, in the third place, this salt, which is so capable of accomplishing the metamorphosis, not only of ordinary effete tissue and mal-assimilated food, but that, also, of many of those products of low vitality which either constitute, or are the manifestations of, a *materies morbi*,—this salt, I say, thus potent to alter and renovate, is itself altered in its progress through the laboratory of the organism, it being well known that the salts of the vegetable acids are all converted in the system into carbonates, and as such are found in the urine. Now, I think it is susceptible of proof, having the strength almost of mathematical demonstration, that the benefit derived from the acetate of potash in such diseases as those I have spoken of, is owing substantially to its being converted into the carbonate of that alkali. But, before adverting to this change which the acetate of potash undergoes in the system, there are some circumstances connected with the action of the carbonate of potash that require to be noticed at this stage of our inquiry. That the carbonate of potash



can exercise a transforming influence over organic compounds out of the body is undeniable. Thus, albumen, digested with an alkali, is broken up into secondary compounds, yielding leucine, formic acid, and the bodies allied to gelatine, called protid and erythroprotid. In like manner casein is broken up into tyrosin, leucine, valerianic acid, and other substances. These being some of the effects which this salt produces out of the body, is it too much to presume, with Dr Golding Bird, "that, when we cause an alkaline carbonate to circulate through the blood, it exerts an influence on the nascent elements of those matters less highly influenced by life allied to that which it exerts on dead matter, aids their resolution into substances allied to those produced out of the body, and actually causes the matter to assume so soluble a form as to allow of its ready excretion?" This notion may be objected to as savouring of materialism, and as tending to lower the controlling influence of the vital principle from its high place in the creed of the physiologist as a cardinal doctrine of his faith. This, however, is not my intention; for every one knows that he has "that within" which laughs at the skill of the chemist, and defies the power of his most searching re-agents; and that, so long as the body is endowed with the vital principle, it can resist, in manner most mysterious, those humbling alterations which so speedily ensue when, separated from its nobler associate, it becomes the subject of resistless change and elemental resolution. The laws of chemical affinity can effect great changes—on dead and lowly vitalised bodies most marvellous changes—but they are not omnipotent; for where, let me ask, is their potency, when the acid gastric juice is not permitted to prey on the living stomach, and what power is it, but life itself, which gives to that otherwise unprotected organ a perfect immunity from such a corrosive fluid? But, while all this is willingly conceded in regard to the more perfect structures, I think it can scarcely be doubted that the vital force is not the sole agent in producing what may be called the chemical phenomena of life; or that, on bodies of low vitality and imperfect organisation—such as the noxious matters which are produced in the system by disease—the laws of chemical affinity, or of chemical tendency rather, as Dumas expresses it, are uncontrolled in their operation and effects. If proof were needed to establish this point, it might be derived from the circumstance, that many compounds which, at one time, were supposed to result entirely from unseen processes in the laboratory of nature, and to be incapable of being formed by any other, are now elaborated with ease by the manipulations of the chemist. Need I refer to the successful imitation of nature in the production, by artificial means, of urea, valerianic acid, &c., in such forms that the artificial are undistinguishable from the natural?

But the objection may assume a more practical shape, and it may be urged, if the beneficial results spoken of be owing to the circulation of the carbonate of potash in the blood, why not administer the



carbonate from the first, rather than trust to its production in the system from the decomposition of the acetate? The answer to this objection is found in the fact, that the exhibition of the carbonate as carbonate does not produce the effects which follow the administration of the acetate. No doubt the urine may be rendered alkaline by the use of the carbonate, and the aggregate quantity of the excretion may be somewhat increased—in short, the carbonate may alter the reaction of the urine, may act occasionally as a renal hydragogue, but very inefficiently as a renal alterative.<sup>1</sup> The effects of these salts, and the theory deducible from these effects, may be thus expressed in syllogistic phraseology, slightly modified. The acetate of potash increases the solid constituents of the urine; the acetate of potash is converted in the system into carbonate of potash; the carbonate of potash does not increase (materially) the solid constituents of the urine; *ergo*, the effect on the urine of the acetate of potash is due to its conversion into carbonate. The inquiry being thus narrowed, and reduced, as it were, to a focus, it falls, I think, within the scope of legitimate inference to conclude, that the physiological action and therapeutic efficacy of the salt are connected in some manner—unknown, I admit, at present—with the metamorphosis which takes place in itself. With the view of elucidating this matter a little, it may be advisable to look at the chemical composition of the acetate, and the difference which exists in that respect between it and the carbonate. Putting the base out of view as being merely allied to a different acid, we find that acetic acid is composed of—



and that carbonic acid is composed of—



Now, as the hydrogen and oxygen are in equal proportions in the acetic acid, it is clear that these elements will unite to form water, and will leave, consequently, four atoms of free carbon to unite with any body for which, of course, it has an affinity. But we are not left in doubt as to the individuality of the body with which the carbon combines; for we find that carbonic has been substituted for acetic acid; and we know that this could only have resulted from the union of carbon with oxygen. Nay more, we know, not only that the carbon has united with oxygen, but we know, further, that

---

<sup>1</sup> While conducting a series of experiments, with the view of establishing this point, my zealous clinical assistant, Dr James Wallace, observed in two cases that the urine contained copious deposits of the triple phosphate, and, what is singular, that these appeared immediately after the use of the acetate had been stopped. These deposits were probably owing to the urine, now diminished in quantity by the withdrawal of the salt, being no longer able to hold the phosphate in solution. Be this the explanation or not, the observation of such deposits is of practical interest, and imposes the necessity of examining the urine microscopically in all cases when giving the acetate, and immediately to intermit its use when a phosphate makes its appearance.



eight atoms of oxygen must have been furnished to convert the four equivalents of carbon of every atom of acetic acid into carbonic acid. But these changes which acetic acid undergoes will be made more apparent from the subjoined diagram :—

CONVERSION OF ACETIC ACID INTO CARBONIC ACID AND WATER.

1 eq. Acetic Acid, . . .	C <sub>4</sub>	O <sub>3</sub>	H <sub>3</sub>		4 eq. Carbonic Acid, . . .	C <sub>4</sub>	O <sub>8</sub>	—
8 eq. Oxygen, . . .	—	O <sub>8</sub>	—		3 eq. Water, . . .	—	O <sub>3</sub>	H <sub>3</sub>
<hr/>								
Total, . . .	C <sub>4</sub>	O <sub>11</sub>	H <sub>3</sub>		Total, . . .	C <sub>4</sub>	O <sub>11</sub>	H <sub>3</sub>

It becomes a matter of great interest, then, though unfortunately one of great difficulty also, to ascertain whence this large amount of oxygen is supplied. Any opinion, however, on this point, must, in the present state of our knowledge, be purely conjectural; yet, at the risk of appearing presumptuous, I shall hazard a few speculations on the subject, which, though they may not carry conviction, will, I hope, excite inquiry, elicit discussion, and perhaps conduct to knowledge; for, to use the beautiful language of Bulwer, it should ever be remembered that the agitation of thought is the beginning of truth.

There are, I think, only three sources whence the oxygen can be furnished—from the food, the atmosphere, or the organism, including, by this last term, all the products, normal and abnormal, which exist for the time being in the system. As, for obvious reasons, there was no great likelihood of the oxygen having been derived from the food of those patients whose cases have formed the basis of these observations, I pass at once to the consideration of the second and third named sources. It is the opinion of Liebig, that the salts of the vegetable acids, which are converted in the system into carbonates, derive the supply of oxygen necessary for that purpose during respiration. This, however, is given, if I mistake not, merely as an opinion—certainly not supported by demonstrative evidence, and, if I recollect rightly, not even by a reason *why* these salts should appropriate oxygen in their passage through the lungs; and, with all deference to so great a master in science, while no proof is afforded in favour of the opinion, there are one or two circumstances which, I think, are entitled to consideration as militating against it. In the first place, the speculation assumes that the decomposition and reconstruction of these salts are effected in the lungs, and leaves out of view the more likely influence of digestion in effecting such changes. Again, this opinion presupposes a greater activity of the respiratory process while such salts are making the transit of the lungs—an event not appreciable, certainly, by any increase in the number of the respirations. And, if more oxygen be not withdrawn from the air, and it be maintained, notwithstanding, that the supply of the gas necessary for converting acetates, &c., into carbonates, is furnished during respiration, then the advocates of that opinion are shut up to the conclusion that, in order to effect the alteration of certain salts, some part or parts of

the organism are deprived of a portion, or, for anything we know to the contrary, of *all* their oxygen—a procedure scarcely in accordance with the general tendency of nature's operations. A theory at least as plausible, I conceive, as this, and one as much in unison with ascertained facts, can be constructed, by supposing that the necessary oxygen is derived from the organism itself. That the effete portion of the organism is being continually disintegrated and resolved into a variety of new forms in the capillary laboratory of the system, is well known and undeniable; is it too much, therefore, to suppose that, amid these ever-varying transitions, a vegetable acid may be broken up and altered, by being brought within the sphere of nascent oxygen, evolved from compounds yielding to the resistless influence of the secondary destructive assimilation? This, however, is all theory—and, probably, very erroneous and untenable theory—and, therefore, though agreeing with Dr Whewell, that “false theory often proves more advantageous to science than the absence of theory,” I shall descend from the airy region of speculation, and leave to others the task of solving this interesting problem in the chemistry of therapeutics.

I have thus submitted evidence, that the acetate of potash is a valuable agent in certain cutaneous diseases; I have also proved that it increases the water of the urine, and its solid constituents likewise; have shown that it is itself converted into a salt, which dissolves and transforms tissues which are but imperfectly organised; and lastly, I have endeavoured to show, that the beneficial effects of the medicine are in some way, yet unknown, connected with the change which is effected in itself. Whatever opinion may be entertained of the theoretical conjectures I have hazarded, I trust my readers will not overlook the practical observations which have been presented likewise; and, in the hope that these will be submitted to the test of experience, I respectfully urge them on their consideration.









